transplanted for a time in an artificial nutrient medium or even in serum from the animal to be grafted, allowing thus a preliminary adjustment to the new environment, but have had no opportunity to give such methods a trial. They are mentioned as possible suggestions for some one who may be able to attack the problem fully equipped with a knowledge of the principles governing immunity and anaphylaxis.

This investigation has been carried out in the Bussey Institution with assistance from the Carnegie Institution of Washington.

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Nutrition and Sex Determination in Rotifers

In an interesting paper in the August, 1913, number of the *Journal of Experimental Zoology,* Claude W. Mitchell communicates a series of observations and experiments upon the rotifer *Asplanchna,* from which he draws conclusions at variance with those hitherto advanced by investigators who have worked with *Hydatina.* His main conclusion, it appears to me, is that "qualitative and quantitative changes in nutrition will be found the universal sex-controlling factor in this group" (rotifers). If it be granted that other factors than nutrition also play the same role in sex determination in one rotifer as in another, I think it may be shown that Mitchell's experiments are not calculated to prove his contentions.

There is, in the first place, some obscurity in the use of the word "nutrition." By the earlier workers on life cycles in rotifers and daphnians, nutrition was measured by the quantity of available food. The rate of reproduction gave a key to the degree of nutrition, but the rate of reproduction was supposed to be proportional to the amount of available food. It is obvious, however, that nutrition may be measured by the quantity of food that an organism can *assimilate,* which may be independent of the amount *available.* In rotifers, for example, there are periods in which reproduction and growth are rapid, alternating with periods in which these processes are slow. Mitchell does the service to emphasize this "physiological rhythm." Rotifers in the period of rapid growth will live well under external food conditions that would reduce rotifers in a period of depression almost to starvation.

When we say that nutrition determines sex, what meaning do we put upon nutrition? One might assume that Mitchell regards nutrition and physiological "level," to use another term of his, as synonymous, were it not that in the seventh paragraph of his summary he lists them separately. To quote:

> Maximum male production is determined by three factors, physiological rhythm, high nutrition and starvation during the growth period.

If nutrition means the quantity of food available, the evidence in its favor as a sex determinant is so small as to be negligible. The experiments of Mitchell do not prove its effectiveness in *Asplanchna,* as I hope to show below, and my own work on *Hydatina* is not only distinctly against it, but explains away the positive results of Nussbaum. If nutrition means the quantity of food that can be assimilated, then high nutrition is probably the result of an antecedent physiological change that is not nutrition at all. Rhythms of reproduction and growth occur in *Hydatina,* in protozoa, in *Cladocera,* and perhaps many other animals; but so far as I know, the physiological change preceding a wave of rapid growth has not been discovered. It may be a chromosomal change. If the wave of rapid reproduction is accompanied by a wave of many male producers, it seems to me we are much more justifiable in assuming that both high nutrition and male production are here the result of some other physiological factor, than in holding the male production to be a result of the nutrition. That the evidence of high nutrition comes earlier in a series of generations than does the evidence of male production may be due to the fact, true at least for *Hydatina,* that sex is determined a generation in advance without any visible sign of such determination. I revert to this point,

apparently overlooked by Mitchell, below in another connection.

If my interpretation of physiological rhythm be correct, as outlined in the preceding paragraph, nutrition and male production stand in the relation, not of cause and effect, but of two effects of some cause. If this interpretation is correct, high nutrition and male production are not inseparable; and there is evidence that they are separable. Early in my work on *Hydatina* I noticed that periods of abundant male production were also periods of rapid growth (the fact which Mitchell emphasizes for *Asplanchna*), and I was almost convinced that anything which increased metabolism would also increase the proportion of male-producers.[1] But in healthy lines I later found that long periods were passed through in which the rate of growth and reproduction was very rapid, yet not a single male-producer appeared. In one instance, there were twelve successive generations in which no family comprised less than 46 daughters, some of them over fifty, which is almost the maximum of all my records. At the same time the females laid 16 to 22 eggs per day, depending on temperature, quite as rapidly as in the waves in which I had previously noted large numbers of male-producers. Yet not one male-producer appeared in these twelve generations. Hence, when actual counts were made from numerous families, for the purpose of proving that rapid metabolism and male production were interdependent, that thesis could not be established. While periods of many male-producers were on the whole periods of rapid metabolism, not every period of rapid metabolism was a period of many male-producers. Rapid metabolism could occur without abundant male production. One is driven, it seems to me, to the conclusion that when male production and rapid assimilation ("nutrition") occur simultaneously, both are probably effects of one cause; but that rapid assimilation may have other causes which do not at the same time cause abundant male production.

Mitchell does not, however, rely wholly upon the high nutrition which accompanies physiological rhythm to explain male production. The "nutrition" which depends upon the available supply of food is also held accountable; for the author conducts experiments in which the food supply is altered, and obtains what he believes to be positive results thereby. The general conclusion from these nutrition experiments is that "male production follows upon the summation of favorable external and internal conditions, plus a sudden interruption by a nutritive check." This check is starvation. The experiments, however, appear to me, for reasons about to be stated, quite inadequate. For example, one experiment consisted in isolating females from periods of rapid metabolism and from periods of depression, starving their offspring for a period after birth, and noting whether the daughters were male- or female-producers. Each part of this experiment involved only about ten individuals. Notwithstanding great irregularities in the occurrence of male-producers, irregularities which the author admits sufficiently to explain certain exceptions, the ten individuals are considered valid evidence. The apparent lawlessness of the occurrence of male-producers is sometimes astonishing. In *Hydatina,* in an extreme case, two sisters, the fourth and fifth, respectively, in their family, reared under what were aimed to be identical conditions, each produced a family of over forty. One family comprised over fifty per cent. of male-producers, the other none at all. In view of such irregularities, experiments including less than eight or ten generations have in my work been regarded with suspicion, unless the effects were quite marked. If such irregularities in the occurrence of male-producers are found in *Asplanchna,* ten individuals do not form a basis for conclusions.

Furthermore, it is questionable whether starvation can have such an effect *on the individual starved* as to change a female-producer to a male-producer. I have shown for *Hyda-*

[1] So nearly convinced was I that this relation existed, that I expressed the idea before a public gathering at the laboratory of the Brooklyn Institute of Arts and Sciences at Cold Spring Harbor, in the summer of 1909, but never in any published work.

tina[2] that it is irrevocably decided during the growth period of an egg whether the female that hatches from that egg will be a male-producer or a female-producer. This is actually proved, it is true, only so far as the effect of chemical substances is concerned. But I am unable to take comfort in the view that sex is determined at a given moment beyond the possibility of reversal by chemical substances, while it is still open to alteration by other external agents. If sex is determined thus a generation in advance in *Asplanchna,* as in *Hydatina,* the starvation experiments referred to above could not have produced positive results; the starvation should have been practised on the mother of the desired male-producer.

In another experiment Mitchell starves a number of young females for a few hours after birth. The first few daughters in each of nine families are used as controls (well fed); they include six male-producers out of a total of 39. The later daughters of the same families are starved; 51 out of 68 prove to be male-producers. The author attributes the higher proportion of male-producers in the latter lot to the check upon nutrition. But, waiving the objection of a rather small number of individuals, another explanation is at hand. It has been shown[3] from 349 families of *Hydatina,* comprising about twelve thousand individuals, that the first few daughters of a family are much less likely to be male-producers than are the later members. If the same relation holds in *Asplanchna,* the numbers of male-producers obtained in the experiment described are about what would have been expected if starvation had not been practised.

In offering this criticism of Mitchell's work I do so in no carping spirit. It is gratifying to find some one using the excellent material which *Asplanchna* affords in an attempt to solve fundamental problems. I have sought only to show wherein lie the weaknesses of the evidence.

A. Franklin Shull

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[2] Shull, A. F., "Studies, etc., III. Internal Factors Affecting the Proportion of Male-producers," *Jour. Exp. Zool.,* Vol. 12, No. 2, February, 1912.

[3] Shull, A. F., "Studies, etc." I., *Jour. Exp. Zool.,* Vol. 8, No. 3, May, 1910.

THE AMERICAN PHYSICAL SOCIETY

A regular meeting of the Physical Society was held in Fayerweather Hall, Columbia University, New York City, on Saturday, October 18, 1913. The following papers were presented:

"The Vapor Pressure of Metallic Tungsten," by Irving Langmuir.

"The Form of the Ionization by Impact Function, $a/p = f\ (x/p)$," by Bergen Davis.

"Change of State Solid-liquid at High Pressure," by P. W. Bridgman.

"Notes on Some Integrating Methods in Alternating Current Testing," by Frederick Bedell.

"Silvered Quartz Fibers of Low Resistance Obtained by Cathode Spray," by Horatio B. Williams.

"The Critical Ranges A_2 and A_3 of Pure Iron," by G. K. Burgess and J. J. Crowe.

"A Spectrophotometric Study of the Absorption, Fluorescence and Surface Color of Magnesium Platinum Cyanide," by Frances G. Wick.

"Examination of the Omnicolored Screen Plate by Means of Microscope and Spectroscope," by John B. Taylor.

"Relativity Theory—General Dynamical Principles," by Richard C. Tolman. (By title.)

"The Hall Effect in Liquid and Solid Mercury," by W. N. Fenninger.

"An Electrolytic Determination of the Ratio of Silver to Iodine and the Value of the Faraday," by G. W. Vinal and S. J. Bates.

"Effect of Amalgamation on the Contact E.M.F. of Metals," by F. J. Rogers.

"Relativity Theory; The Equipartition Law in a System of Particles," by Richard C. Tolman. (By title.)

"Failure of Color Photography by Commercial Screen-plate Methods for Spectroscopic Records," by John B. Taylor.

"Condition Involving a Decrease of Primary Current with Increasing Secondary Current," by F. J. Rogers.

"Experiments on the Magnetic Field of Two Electromagnets in Rotation," by S. J. Barnett.

"The Effect of Space Charge and Residual Gases on the Thermionic Current in a High Vacuum," by Irving Langmuir.

Alfred D. Cole,
Secretary